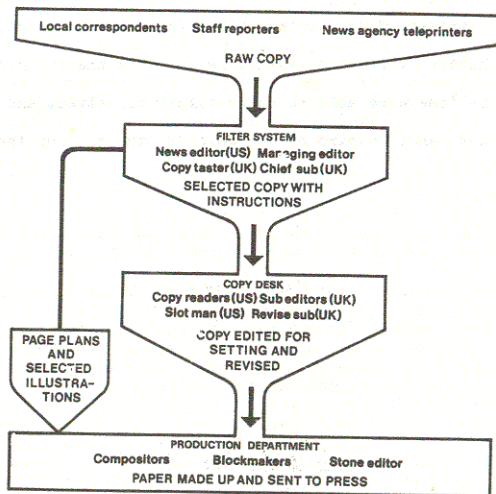


EDITORIAL

As can be seen from the Contents list, this issue is essentially a report on the Twelfth Annual Conference of the Radical Statistics Group, plus some added spice from the ever-productive Health Group. However, tradition has it that the editor should pontificate. Here goes. Given the growing emphasis on 'consumerism' in this enterprise culture, we wonder whether there is room for a critique which moves a little beyond "there is potential distortion" but not quite as far as Gram.sci. Could citizen controlled satisfaction surveys become a major check on the arbitrary exercise of power (see also report from SPG workshop). Two other brief points from the Conference: one attender demanded his money back because we were not radical enough; and we (the editors), at least, were surprised at the payment for personal service. Have we "lost our way"?



The deskman's part
in newspaper production

Rude Comment from Roy CARR-HILL

Reviewing: Class and Health

I realise RSN is a consensus publication eschewing conflict and controversy but I can't let Graeme Bett's extended review of Class and Health pass without comment.

First, whatever else I may say about the book, it is important to single out Mildred Blaxter's review of longitudinal studies not only as an indispensable reference source but also for its important critical discussion of the value of longitudinal studies in this area. Second, let me declare a double Members interest: I presented a paper at the SSRC Workshop which was at the origin of the book and I have reviewed Class and Health elsewhere. Third, I should make clear my substantive interests which are perhaps a little idiosyncratic: I want to understand how poverty affects health; I do not want to shore up the Victorian institution of Class and I believe that, when writing for the RSN, one should at least nod in the direction of statistical rigour.

From these latter perspectives, Class and Health (with the honourable exception of Mildred Blaxter's review) is a poor collection. First, apart from the editor's own mysterious arithmetic on Chapters 1 and 6 (see below) there is very little new data. The data from the two articles from the Social Statistics Research Unit by Fox et al, have been presented in numerous conferences with the argument of the first already published in 1982: and the bulk of Marmot's paper had already been published in The Lancet. Only Wadsworth was relatively new and his results go against the general trend of the book.

Second, the book does not refute the selection hypothesis. Richard Wilkinson's conclusive 'demonstration' that illness has little impact on selective social mobility assumes illness only happens once and assumes that the increased risk of serious illness due to a prior childhood illness is independent of an illness-related downward mobility process, and his calculations to "show" that the selective mobility factor can only account for 10% of differentials in infant mortality rules on a presumption that the relationship between adult health and infant mortality is strictly one-to-one (a 10% difference in adult health corresponding to a 10% difference in infant mortality).

Neither does the book refute - let alone understand - the artefactual hypothesis. The comparison of Classes over time MUST allow for their changing size. These are large (see Table 1). Put crudely, given present trends in

Table 1 Distribution of Economically Active Men by Occupational Class England and Wales 1931-1981

	I	II	III	IV	V	All Classes
1931	1.8	12.0	47.8	25.5	12.9	100
1951	2.7	12.8	51.5	23.3	9.7	100
1971	5.0	18.2	50.5	18.0	8.4	100
1981	5.7	22.3	45.6	15.9	5.8	100

Source: Illsley, (1986) and own calculations from DS (1986)

occupational restructuring (excluding or including unemployment to taste) the ratio of class death rates is almost bound to increase whilst the numbers of deaths involved gradually disappears (see, for example, Table 2).

Table 2 Trends in infant mortality by occupational class

Social Class	Infant deaths per 1000 legitimate live births				
	1930-32	1949-53	1970-72	1978/79	1984
I	32	19	12	10	6.5
V	80	42	31	18	13

Source: Townsend and Davidson, 1982, Table 10, p.71;

Third, Wilkinson's own 'demonstration' of the connection between income and health is a travesty. He makes causal inferences from a comparison of percentages changes in mean occupational earnings and occupation-specific death rates with N=22, a comparison of the level of data pensions and mortality among the elderly with N=16 and a comparison between GNP and national mortality rates with N=11. Not only are these numbers very small, the correlations are at a very macro level.

No-one - even the extreme right - disputes a connection between income and mortality: aggressively piling up that evidence gets us nowhere. The issues are whether low incomes are raised most effectively by redistribution now or by trickling down from the accumulated wealth of the rich and what kind of relationship between income and health (linear/non-linear/threshold; more or less immediate or stretching over the life cycle, etc). We know where we stand on the former dichotomy; but there are important large differences in policy emphasis according to where you stand on the latter. Wilkinson's book gets us nowhere.